Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

THE PENEPLAIN — A REVIEW.

R. A. DALY.

ALL workers in Physical Geography are agreed that, given time enough and a constant position of the baselevel, the general processes of denudation on the land will produce, as the result of wearing down any massif, a nearly plane surface approximately coincident with the baselevel of the region. Professor Davis and his followers believe that this ideal condition is represented by actual examples in nature, examples which deviate from the ideal in features that are the expected concomitants of even long-continued denudation. Chief among these is the occurrence of isolated areas of higher ground than the general plain, existent as such because of greater initial elevation of those parts of the massif or because of their being composed of exceptionally hard rocks; they are the "monadnocks" dominating the otherwise nearly featureless plain. That, in such a case, we have to do with an almost-plain, a peneplain, is regarded as none the less certain on account of the presence of these residual hills; on the contrary, they furnish one of the strongest arguments for the fact of denudation. The nearly plane surface of the rest of the massif can be quite independently recognized in the field and on the map. In a recent number of the *American Geologist*[1] Professor Tarr states his opinion that, while conceding the possibility of peneplains on an ideal planet, they do not exist, have not existed, and, if I understand him aright, *can* not exist on this earth of shifting baselevels, moving up and down within time limits represented by one or more geological periods.

Professor Tarr's arguments against the theory of peneplains are both general and special. In the former category are the following: (1) The theory is faulty because it demands too much time. (2) Closely correlated therewith is the objection that continental oscillations of level are too pronounced, both from the point of view of amplitude and of frequency, to allow of the amount of beveling requisite to reduce a mountain-built region to the faint relief of a peneplain. (3) There is no known case of a modern extensive peneplain standing at its baselevel. (4) The paper is, however, largely occupied with a concrete argument against the peneplain from an analysis of the facts of land-form in two of the classic regions where the peneplain was first described, namely, in New England and New Jersey.

---

[1] June, 1898.

Before proceeding to a detailed discussion of these various points it is necessary to observe that, in order adequately to demolish the theory, it must be inspected, not only in the writings of the American physiographers, but also in those of European authorship. The peneplain is an American idea, but it has taken firm root in Europe. Men like Penck, Philippson, De Margerie, and De Lapparent are recognizing its truth, and we read of the peneplains of Bohemia, Russia, and the Rhine district; yet we cannot say that these men are specially omnivorous of American geological and geographical conclusions. We may most heartily agree with Professor Tarr in his calling a halt on the wholesale discovery of peneplains on insufficient evidence, but that criticism does not apply to the examples cited.

1. Professor Tarr refers to the difficulty of finding time enough for the process of the peneplanation of a mountainous tract, from the fact that, since the glacial period, there has been sufficient time neither to "strip off the till left by the ice upon the hillsides [of New England], nor to notably modify the very perfect form of drumlins, eskers, and deltas formed when the ice was here." "When we see the slowness of denudation in a hilly country, even a single peneplain seems most difficult to conceive." Is it easier to *conceive* (*i.e.*, actually measure out in the imagination) a stretch of time long enough even to permit of the excavation of the Colorado Canyon or the retreat of the Niagara escarpment fifty miles or more from the former edge of the Niagara limestone, or to produce the bewildering network of valleys in the West Virginia plateau? Whether we

can conceive many geological processes or not does not alter the facts of geology. The velocities of the planets are real, though never adequately conceived by the astronomer. The work has been done — "How?" is the question. To reason truly and effectively about the genesis of a land-form, we must put away from our minds any proneness to measure the scale of the operations by reference to the standards of a human life. It is safe to say that the best work in geology has been done by those observers who have thus put themselves in a sympathetic relationship with the earth and have looked upon her as a great organism whose age is to be evaluated in proportion as her activities become known. The culture of the imagination is an academic bi-product. This argument of Professor Tarr would, then, as seriously militate against *any* important modification of the lands by denudation as it does against that last stage where the forces of erosion have carried it down to a condition approaching a plain. The same criticism can be applied to his illustration of the slowness of geologic changes taken from the behavior of the Penobscot River in time of summer freshet. It is true that there is but little sediment in the running water of the stream, but does Professor Tarr deny that the neighboring mountain, Katahdin, has been worn out of an enormously greater terrane than that represented in the present mountain-root? We may note, in passing, that in any case the actual work of the Penobscot cannot be gauged by mere observations on a summer freshet; it is in the spring, after the frost has loosened débris from the mountain-sides, that most of the transportation is effected for the year.

2. The second objection referred to is more concrete and withal more scientific; yet it may be met in the same way. Each of many of the greater unconformities in the geologic scale means a stand of the land long enough to remove almost completely a mountainous relief of surpassing magnitude by the self-same process of slow denudation as that characteristic of the lands to-day. Witness the pre-Palæozoic land surface of the crystalline shield of Canada, the wonderfully even surface of the Archæan underlying the fossiliferous rocks of European

Russia or of the plateaus of the Colorado Canyon. A striking example of these buried plains of denudation has recently been seen by the writer in that treasury of physiographic illustrations, the Crimea. As the steamer coasts the southwest shore of the peninsula, one sees a dark line running through the cliffs with almost ideal straightness for many miles; this line represents an old land surface on the well-flexed Jurassic, in which the steeply dipping beds are truncated as nicely as if pared down by a huge knife. Upon them lie the light-tinted Eocene sediments, horizontal, and signifying a perfect case of unconformity. Now we believe that this smoothing of the older terrane in each case is capable of explanation, and as yet only two theories have been advanced to serve the purpose. The first, the theory of subaerial denudation, has by far the weight of evidence in its favor; the second, that of marine denudation (sea-benching on a large scale), is in the highest degree improbable, for reasons that need not concern us here. Both theories alike demand a fairly constant relation of land and sea, not absolute quiescence from up and down movement of the land, but comparatively faint oscillations of the land during a long period. If we are going to explain these fossil land surfaces at all, then we are forced to posit a more or less constant baselevel for each. We have, furthermore, in these same regions positive evidence that they have since been capable of a long-continued absence of important movement of the land with respect to their common baselevel, the level of the sea. As far as the wild Archæan tracts of Canada are known, it seems to be the fact that at least one million square miles of that country have remained above the sea during most of Palæozoic time and all of Mesozoic and Tertiary time. Conversely, from the Cambrian to the Triassic, the Russian terrane was sunk below the sea.

3. We must again join issue with Professor Tarr, when he contends that there are no extensive peneplains of modern date existing on the earth. The Russian Plain, from St. Petersburg eastward and northward, is essentially in this condition; while, considering its vast extent, the whole of the European part of the empire has suffered but a minimum amount of warping from

the original position of the same peneplain. But, granted that there be no recent peneplain at or near the present sea level, it would not prove that such plains have not been formed in past geologic ages. Late Tertiary time has been that of exceptional energy in mountain-building on the lands, and very probably in drowning of parts of the continental ridges in the ocean. Both of these correlated causes might well bring about an important lowering of the sea level simultaneously about all the continents. The result of such movements would be to raise some of these plains above their ideal position near the sea, to tilt others either toward or from the sea, and, possibly, to leave one in about its original attitude with respect to the ocean level.

4. Professor Tarr goes farther and denies that there has yet been adduced evidence that there exist peneplains of ancient date, *i.e.*, those which have been uplifted and are being dissected in a new cycle of geographic development, or those that have been buried in sediments after depression; the peneplain theory is useless because there is no peneplain to need explanation. To establish this doctrine he gives us the results of his study of the uplands of New Jersey and New England.

He finds five hundred feet of difference in the tops of the New Jersey hills, and about as much in "the very even-topped Kittatinny Mountains." "There is a very distinct lack of uniformity in the elevation of the upland crests"; it is truly a distinct lack, but is it, even on the showing of Professor Tarr's figures, sufficient to remove the topographic facet represented in the average level of these higher points from the category of an almost-plain? He tabulates the ranges of elevation on the survey sheets of Connecticut as follows:

Cornwall	1787–1215	feet, a	range	of	572 feet.
Winsted	1600–1160	"	"	"	440 "
Granby	1240–720	"	"	"	520 "
Hartford	lowland.				
Tolland	985–660	"	"	"	325 "
Woodstock	761–540	"	"	"	221 "

He concludes that it is more important to emphasize the total range of elevation of the crests in this 91 miles than to emphasize the fact that in any given sheet the total range is relatively very small and decreases with the decreasing absolute heights of the hills in going from west to east. Is this striking accord of elevations to be ignored in an explanation of Connecticut scenery? Professor Davis has regarded it as indicative of a peneplain of the last geographic cycle, now tilted towards the east and somewhat dissected, especially on the softer rocks, in the present immature stage of a new cycle. It is a theory at once clear, definite, and involving no processes other than those actually illustrated on the earth at the present time. It explains the existing uplands and valleys of the regions under discussion, and, above all, the presence of this otherwise inexplicable topographic facet which is once more established by the foregoing table of Professor Tarr.[1]

A second point that he makes against the New England peneplain shows a disregard of patent facts which it is most difficult to understand in the most superficial survey of New England geography. He writes: "In Maine, New Hampshire, Vermont, western Massachusetts, and the Adirondack region, with similar structure to that of the region above mentioned, and so near them that they must have been subjected to the same general degradation, the lack of uniformity of upland crests is very much more marked." The elevation of the latter "is 2000 to 4000 feet above the sea." Now, so far as the present writer is aware, neither Professor Davis nor any one else has contended that these summits represent any part of the peneplain; they are, on the contrary, stated to be local and regional monadnocks interrupting the general surface of the peneplain for very good reasons. Among the latter is one which has met with contradiction in an expression of the passage just quoted, namely, that there is similarity of "structure" between these terranes and that underlying the peneplain. Whether "structure" means "position" alone or "position" plus "texture" (as it manifestly ought not to do), we cannot agree with the statement. The granites, granitites, syenites, and quartz porphyries of Osceola, Tripyramid, the eastern Kearsarge, Red Hill, and the New Hampshire Whiteface, are entirely different in structure and mineralogical characters from the foliated rocks of Massachusetts or even the batholites of coarse porphyritic granite of New Hampshire and Massachusetts, and we believe that no practical worker in the field relations of these areas finds difficulty in putting in similar contrast the Massachusetts foliated rocks and the heavier, more massive gneisses and schists of the White Mountains proper. Is the structure of the Adirondack granite massif "similar" to that of the Berkshire plateau, the structure of Mount Ascutney, Vermont, with its three great stocks of deep-seated intrusives, "similar" to the structure of the surrounding phyllites and gneisses, or that of the granitic Katahdin "similar" to the Calciferous slates through which the granites came?

---

[1] I should not hesitate to speak of an extensive mountain range as a "peneplain" if it no longer showed a range of elevations greater than 572 feet in a stretch of 91 miles. It is, however, true that the ranges of elevation cited by Professor Tarr are determined from the position of streams which have strongly incised their beds because of the Tertiary uplift. On the other hand, some of the higher points may represent extremely low monadnocks overlooking the peneplain of Professor Davis's definition (range of elevations through 200 or 300 feet) from altitudes of 100 or 200 feet.

Professor Tarr goes on to say that he has stood on the "higher" peaks of Maine and looked in vain for any series of peaks that even to the eye appeared uniform in level. For any one to advance this observation as an argument against the New England peneplain seems to us quite incomprehensible. To look for a fairly chosen peneplain sky-line a thousand feet or two thousand feet above the level at which the advocates of the peneplain ask us to find it, and to look for that sky-line in a nest of granitic monadnocks, is hardly likely to be the mode of procedure by which to successfully attack the theory of the peneplain.

He questions any serious lack of "sympathy between the level-topped hills and the rock texture and position." He notes the fact that the soft gneisses and the limestones of New Jersey lie in regions of average lowland, and the hard gneisses underlie areas of greater elevation. This is the sole argument he makes to support this lack of sympathy. He then states, without the shadow of proof, that, "although the rocks are complex in kind and position, they now stand in very general harmony with topography." In the next paragraph he admits that "the region is a lowered mountain mass, evidently once of a very rugged topography, but now much reduced and traversed by drainage lines of a somewhat mature form, the result of elevation." But that there be or not a lack of sympathy between structure and land-form will evidently depend, among other conditions, upon the degree of the lowering. The make-up of the New England upland certainly indicates the former existence in the district of massifs of flexed rocks at least as imposing from their altitude as the modern Alps (one resulting conclusion from the detailed work of Emerson in western Massachusetts, of Shaler and Woodworth in the faulted and folded region of Narragansett Bay, of Pumpelly, Wolff, and Dale in the Green Mountain axis, to say nothing of the older work of the Hitchcocks and others). Where are the ridges corresponding to the folds of the Boston Basin, or the fault-scarps of Rhode Island or of the Connecticut Valley? It is, in other words, impossible to speak of sympathy between the topography and the structures of New England, when the latter demand displacements of thousands of feet, especially in view of the fact that the valleys and hills of the upland run at all angles to the axes of folds or the lines of fault. There *is* some sympathy between topography and the hardness of the rock-members, but it is only with the extremest rarity that there can be traced any correspondence of structure (position) and the land-form in New England outside of the Narragansett Basin or the Triassic lowland.

The evident adaptation of many of the valleys and broader lowlands of New England to the superior softness of the rocks they cover is just what we should expect to find on any theory of New England topography that recognizes a period of uplift, recent, but long enough ago to allow of the excavation of the valleys to their present levels. This uplift Professor Tarr acknowledges, but claims that "there is no evidence to prove" that this uplift has been differential, that is, that there has been a tilting toward the southeast. If there has been no such tilting, it is necessary, following Professor Tarr, to consider that while the Deerfield has cut its canyon-like valley, and is still cutting downward with energy, the eastern part of Massachusetts has been lowered to a degree where, in Professor Tarr's words, "there may well have been an approach toward the condition of a local peneplain." That is, the relatively insignificant amount of work necessary to hew out the Deerfield valley must have taken as much time as the wasting to an almost-plain of a coastal belt from twenty to thirty miles broad at least. From his own point of view, Professor Tarr would explain this strong contrast in the rate of cutting by the fact that the coastal region is the locus of river systems with short courses to the sea, and, consequently, great head and erosive power. But how is it that the plateau of western Massachusetts is not similarly worn down by the rivers which have a quick course to the sea *via* the long-opened-up Triassic lowland on the west? (This implication, in the last quotation, that subaerial erosion may produce a peneplain near the coast on the scale permitted by the range of elevations in Massachusetts, seems to be a virtual abandonment of his whole position on the part of Professor Tarr.)

The theory of differential uplift is antecedently probable, and the facts of present geographic form are exactly those that would be expected to result from the elevation of New England at the close of a former complete cycle of denudation. A tilting toward the east and south would give the revived rivers of the northern and western parts of the peneplained region power to etch out of the terrane narrow valleys on the hard rocks and broader valleys on the less resistant ones. The broadening of the valleys would progress as the time allowed, as the area of soft rocks permitted, and as the river was strong. Where the second and third conditions are satisfied, as in the case of the Connecticut, we have a broad open (Triassic) lowland of the new cycle; when the second is not so well fulfilled, the same river gives us a much narrower lowland, that on the "Calciferous mica-schists" of southern and central Vermont.

One of the most strongly emphasized objections of Professor Tarr to the theory of peneplanation is based on the occurrence of monadnocks, or residual hills, on the New England upland. He asks: "Are the monadnock rocks essentially harder than the other hilltops of the neighborhood?" and answers the question in the negative on page 363 of the article under review, and in the affirmative on page 369.[1] If an affirmative answer be not possible, there is no possible explanation of these higher crests in the light of our present geological knowledge. Monadnocks like the Blue Hills near Boston, Ascutney Mountain, Chocorua, Katahdin, are made of plutonic rocks, necessarily crystallizing under the pressure of overlying masses of the same lithological character as the rocks now seen in the immediate vicinity of these eruptive centers. The overlying rocks are gone, the surrounding rocks are well beveled down; the stock-rocks stand up as knobs — Why? — because they are harder. If there can be suggested a simpler, more probable explanation, it is high time that geologists should find it out.

It has been my fortune to map geologically one of these typical monadnocks, Mount Ascutney, and, on all sides of it, to meet with striking illustrations of the fact that it stands above the general level solely because it is more resistant to destructive agents. The streams running across the syenites and granitite far up the mountain are slowly cutting their gorges, continued in radial arrangement, across the contacts on all sides. At the contacts there is almost universally an abrupt fall where the streams escape on to the thinly foliated schists or basic intrusives that encircle the main mountain. This exhibition of differential hardness is correlated with an only less important steepening of slope where not the perennial streams, but the general process of wasting and wash, "creep," has developed a sudden fall-off just at the zone of contact of intrusive and country-rock. In the reconnaissance of the mountain this latter slope was used as a rapid means of locating the contact.

Now *these* are the test cases. It is, indeed, difficult to say why the gneissoid and schistose rocks of Mount Monadnock have refused to weather down as rapidly as the similar schists round about; but, by all the laws of analogy, it is simplest to believe that the same explanation of differential hardness applies here. (I would go so far as to say that, on the basis of direct observation in the field, it is not differential *elevation* that explains the existence of Mount Monadnock.)

---

[1] On page 363 we read: "I believe that I am correct in saying that there are no very distinct differences between the rocks of the monadnocks and the lower hills, in point of durability." On pages 368 and 369 we have: "There will be a beveling of hilltops where the harder gneissic and granitic rock exists, the stream valleys standing near the base level, and hills of softer strata standing at levels still lower, in which the rock is harder. . . . Can any evidence be adduced to show that New England has ever advanced further in development than this stage?"

Following his criticism of the peneplain theory, Professor Tarr attempts some constructive theorizing on the present relief of such regions as New England and New Jersey. He regards it in each case as a mountainous area in the condition of "full maturity of topography," "reduced mountains, lowered to the stage of full maturity." "By this explanation it is held that the region was never reduced to the peneplain stage, but has always been, as it still is, a mountainous section, though once less mountainous than now, because of the recent uplift." But, disregarding the uplift, is it correct to speak of such relief as that of Massachusetts as characterized by a stage of "mature" dissection? Will the average inhabitant of the upland be flattered in learning that the tract in which he lives ought, by the laws of distribution, to be occupied by a few miserable, impoverished individuals, isolated by a labyrinth of valleys and hills from their fellows? For such is the condition of most of the maturely dissected areas of the earth's surface; the moonshiners of West Virginia eloquently represent one result of mature dissection. Fortunately for the needs of man, a maturely dissected region is almost as rare as one that is characterized by chronic earthquakes of destructive violence. But when, in addition, Professor Tarr states that, by "mature" topography he did not mean even the present relief, but a topography made "more rugged" by the recent uplift, it is all the more difficult to subscribe to his nomenclature of that earlier surface. So far is the Berkshire plateau, for example, from being of "mature" form, made more rugged by uplift, that an unprofessional observer, returning from a walking tour across the plateau, has remarked to me in very definite language that the plateau is "flat," so strong was the impression upon him of the even-topped character of the whole. Yet how likely is the untrained student of land-forms to overlook the existence of a topographic facet, however faintly it may be dissected!

The most valuable part of the paper is that in which Professor Tarr emphasizes the effect of the tree-zone in retarding the progress of denudation and the consequent tendency of the higher summits of a mountain range. Pike's Peak is a good example in point, the rounded slopes below that zone contrasting finely with the serrate spurs of rapidly degrading naked rock far up. Professor Tarr would find in the halting of one peak at the snow-line until overtaken in the downward journey by its neighbors an explanation of the even tops of "mature" mountains, in particular those of New England. But the very rough accordance of levels expected in this ideal scheme, and represented in parts of the Alps and Carpathians, is not in the slightest degree comparable with the New England conditions, nor with the "very even-topped Kittatinny Mountains," referred to in the article. Thus it is impossible to conceive that the New England facet can be accounted for as formed at a vanished tree-zone. Having reached the tree-zone, the forces of denudation then tend to reduce the mountains still further, but now according to the law of soft and hard. Now, since the effective tree-zone may be roughly put at an elevation of between 4000 and 5000 feet in New England, we are as badly off as ever in arriving at a conception of how a nearly plane surface can be produced on the rocks of differing hardness composing the mountains, unless that surface is the result of the beveling down of all the terranes nearly to baselevel. Finally, there are well-established peneplains, such as that on the old rocks of Missouri, where the tree-zone theory cannot avail, because the district was undoubtedly never raised, at any time during the geographical cycle represented, to a height that would bring about the differentiation of a tree-zone. It is an old plateau of horizontal or but gently folded rocks.

HARVARD UNIVERSITY, December 10, 1898.